tending to make their preservation difficult, which may perhaps be taken into account.

Some kinds of coniferous plants resist decay, when immersed in water, more completely than do almost any dicotyledons, and this resistance may, owing to their resinous nature, be very greatly increased when the immersion is in sea-water. This supposition is borne out by a fact I have noticed, that in some Eocene beds, such as the marine beds at Bournemouth, the Bembridge marls, the Bracklesham beds, coniferous remains preponderate, whilst from the two latter places I have never seen remains of dicotyledons at all, although there is evidence in these cases that dicotyledons were abundant on all surrounding land areas. This may partly account for their complete absence in marine cretaceous rocks in England, where, as in the gault, &c., foliage, fruit, resinous gums in the form of amber, remains of coniferæ, are preserved. The foreign cretaceous rocks, in which an abundance of dicotyledons is met with, are principally of fresh-water origin.

It should be borne in mind that our Chalk period contains a deep sea fauna, and we have no record in England as to what were the prevailing contemporaneous shallow water forms of life in other regions. I have great doubts, however, as to the correct position of many of the foreign so-called cretaceous beds. Those of America, from which most of the list of dicotyledons of this period is derived, appear to me, from the character of their fauna, to be either Lower Eocene, or at most filling in the gap between our chalk and London clay. Most of the shells have a marvellously Eocene-like aspect, and I take it that the presence of an ammonite, and some few other forms of shells, which in England do not range above the Chalk, should not be taken as conclusive evidence of the antiquity of the bed, as although migrated from our seas, they may very well have lived on in other regions. It is inconsistent to assume that no ammonite lived on in any part of the world to a more recent period than that of our Chalk; the finding of pleurotomaria and other supposed extinct cretaceous shells in Australian waters, should not be forgotten. The same doubts apply to many of the European leaf deposits; many of these are isolated patches, and their age has been inferred rather from the character of the leaves than from their stratigraphical position. The age of many of the so-called Miocene leaf-beds is admitted now to be extremely doubtful.

What little evidence we may expect to find in these beds seems to me likely to be in favour of the theory of evolution by descent, although until the flora has been worked out, it is premature to offer an opinion. By far the greater number of the plants belong to the lowest division of the dicotyledons, the *apetalæ*, a minority are *polypetalous*, whilst none can, as far as I know, with certainty be assigned to the highest (according to Haeckel) group, the *monopetalæ*.

Prof. Ettingshausen has traced the gradual development of some of the Miocene forms into existing species, notably that of *Castanea atava* to *Castanea vesca*; when he was here last summer and saw my collection, he especially picked out the castanea from Bournemouth as carrying the history of this genus a step further back, and linking it with the oak—as it possesses an oak-like character of venation. I would merely add that many botanists who have studied fossil plants, as Unger, Schimper, and others, are profoundly impressed with the amount of botanical evidence that has already been brought forward in support of the theory of evolution.

OUR ASTRONOMICAL COLUMN

Red Star in Cetus.—No. 4 of Sir John Herschel's list of red stars at p. 448 of his Cape Observations is placed by him in R.A. 1h. 19m. 8·7s., N.P.D. 123° 26′ 1″ for 1830, with the remark "most beautiful orange red. Two observations," and he estimated it 6m. Dunlop in his catalogue of 253 double and triple stars in vol. iii. of the *Memoirs of the Royal Astronomical Society*, gives the position of a highly-coloured object thus: for 1827, R.A. 1h. 19m. 43s., N.P.D. 123° 31′, and calls it "a very singular star of the seventh magnitude, of an uncommon red purple colour, very dusky, and ill-defined;" he made three observations upon it, and notes that it had a small star preceding and another following it. We may presume that these stars are identical, with an error of position on the part of one or other observer, most probably on Dunlop's, whose catalogue contains a number of errors; and it may also be supposed that this is the star spectroscopically examined by Secchi, which he calls No. 11 of Schellerup's catalogue of red stars, but places in 35° 17′ S. declination (A.N. 1737), perhaps through a misprint. In this state of uncertainty as to the star's true place, meridional observation appears very desirable. So far we believe it is not to be found in any catalogue, founded on such observations; it does not occur in the zones published in the Washington volumes 1869-71, a most valuable series, nor in those of Prof. Ragona in the *Giornale Astronomico e Meteorologico del R. Osservatorio di Palermo*, vols. i. and ii., neither is it found in the southern catalogues of the Cape, Madras, or Melbourne Observatories. Sir John Herschel's place reduced to 1877·0 is R.A. 1h. 21m. 19·0s., N.P.D. 123° 11′ 16″. Secchi says of the star he examined, "couleur rose; spectre à zones discontinues."

Variable Stars.—There is considerable probability that Lalande 12863-5 should be added to the list of variable stars. His estimates of magnitude are $6\frac{1}{2}$ and $8\frac{1}{2}$; it is 6 on Harding's Atlas and in Argelander, 6·7 in Heis, 7·3 in the *Durchmusterung*, but does not occur in Piazzi, Bessel, or Santini. Piazzi has a star of the ninth magnitude about $1\frac{1}{4}°$ distant (VI. 190), which, oddly enough, he places in the Lynx. The position of Lalande's star for the beginning of 1877 is in R.A. 6h. 35m. 23s., N.P.D. 83° 32·3′.

Will some one of our southern readers record the actual magnitude of μ Doradus? At present we have the following estimates indicating a long period of variation. La Caille 5m. about 1751, Brisbane 6m. about 1825, Jacob 9·5m. in 1850 and 9·2m. in 1855; while Moesta states that between February, 1860, and January, 1861, he had always found it $8\frac{1}{2}$m. or 9m. The law of variation may be similar to that of 34 Cygni, P Cygni of Schönfeld, the so-called Nova of 1600.

A Fifth Comet in 1851.—In a small tract entitled "Ragguagli Popolari sulle Comete Periodiche," by Prof. Ragona, published at Palermo, in 1855, there is reference to a comet stated to have been discovered at Rome by Prof. Calandrelli, director of the Pontifical Observatory, in the morning twilight on November 30, 1851, which both the discoverer and the writer of the tract considered to be the short-period comet of Brorsen, due in perihelion in the autumn of that year. By comparison with B.A.C. 4798, the following position resulted:—

1851, November 29, at 17h. 32m., M.T. at Rome.

Right Ascension, 14h. 21m. 38s. Declination, + 1° 47′ 2″.

This position Prof. Ragona compares with the elements of Brorsen's comet according to Dr. van Galen, and found the differences between calculation and observation + 35′ 27″ in R.A., and + 11′ 1″ in declination. But notwithstanding this approximation, it is certain it was not the periodical comet of Brorsen that was observed by Calandrelli, Dr. van Galen's prediction having been vitiated by a serious error of calculation, so that, instead of arriving at perihelion on November 10, the date assigned by him in *Ast. Nach.*, No. 782, the comet passed that point in its orbit about September 25, and consequently on November 30 was far removed from the position of the body observed by Calandrelli, which was therefore a new comet.

It is stated that Calandrelli published an account of his observations in the Roman journals in December, which was transferred to the official journal of Palermo on the 11th of the same month. Perhaps a reference to the Italian journals might bring to light a further observation or observations; the comet is said to have been bright, but the weather about the date of discovery was unsettled, and for several days previously had prevented observations of any kind.

COLOURED BELTS ON JUPITER.—In connection with the supposed periodicity in the appearance of marked colour on the belts of this planet, the observations of Gruithuisen, of Munich, in the years 1836-40 possess interest. They are found in his *Astronomische Jahrbuch*, 1839, p. 76, 1840, p. 99, and 1841, p. 101. He first noticed the colour on April 23, 1836, at 9½h., when, observing with a 30-inch refractor of 2⅗ inches aperture, and power 150, the single central belt then visible had a brown tint throughout, and he states that, hardly believing his own vision, he called a person who was at hand, and on asking him what colour the belts presented, he replied "the colour of rust." With a 5-feet telescope, power 120, the brown tint was not distinguished. On subsequent occasions he found that with the highest powers of the telescope the belt appeared of a bright reddish brown, while with the lower powers it was merely of a dark shade, and hence concluded that the intensity of light was disadvantageous to discerning the colour. In addition to the brown tint of the central belt, it was remarked that the planet near its north pole had a bluish-grey tint in May, 1836; a few months later Dr. Albert, a pupil of Bessel, observing with a 30-inch telescope, found the polar region "quite blue." The length of Gruithuisen's descriptive remarks prevents their being transferred to this column, but we refer to the observations, as his annual volumes are not often met with here, and the fact of such observations having been made forty years since may not be generally known. That these tints should have been conspicuous with such small optical aid is worthy of note.

THE INTRA-MERCURIAL PLANET.—In M. Leverrier's last communication to the Paris Academy on the planet assumed to exist within the orbit of Mercury, it was mentioned that, with the elements adopted, or very similar ones, a solar conjunction would occur on March 22, and a transit over the sun's disc was possible, though uncertain. A close examination of the disc is therefore to be recommended on March 22 and 23, and there is reason to believe that observers in widely-differing longitudes are prepared to undertake it. If no transit should then occur, eight or nine years may elapse before one is possible at the spring node.

CHEMICAL NOTES

ATOMIC WEIGHTS OF CAESIUM AND RUBIDIUM.—M. Godeffroy gives an account in Liebig's *Annalen* of some determinations he has made on the above subject. To obtain pure material he employs Redtenbacher's method for the separation of the caesium, rubidium, and potassium, by preparing their respective alums, separating these by fractional crystallisation, and finally converting them into pure chlorides of the metals. The determination of chlorine in the non-diliquescent caesium chloride, gave, as the mean of four closely-agreeing experiments, the atomic weight of caesium as equal to 132·557, the atomic weights of chlorine and silver being taken as 35·46 and 107·94 respectively; from analogous experiments the author finds the atomic weight of rubidium to be equal to 85·476.

ON THE SPECIFIC HEAT OF GASES.—In *Poggendorff's Annalen*, clvii., E. Wiedemann gives a most interesting communication on this matter, in which he criticises the experiments of Regnault on the same subject, and describes a new method of determining the specific heats of gases introduced by himself. On comparing the author's results with those of Regnault it is found that the method employed by the former is not inferior in accuracy to that of Regnault, and also that a great economy of material may be effected by using Wiedemann's process; this economy giving the experiments greater range in a comparatively shorter time. The following tables give a synopsis of the numbers and numerous tables given in Wiedemann's paper:—

Specific Heats of Equal Weights.

	I.	II.	III.	IV.
	0°	100°	200°	
Air	0·2389	—	—	0
Hydrogen	3·410	—	—	0
Carbon monoxide ...	0·2426	—	—	0
Carbon dioxide ...	0·1952	0·2169	0·2387	22·28
Ethylene	0·3364	0·4189	0·5015	49·08
Nitrous oxide	0·1983	0·2212	0·2442	23·15
Ammonia	0·5009	0·5317	0·5629	12·38

Specific Heats of Equal Volumes.

	V.	VI.	VII.	VIII.	IX.
	0°	100°	200°	Specific weight.	$\frac{PV}{P'V'}$
Air	0·2389	—	—	1	1·00215
Hydrogen	0·2359	—	—	0·0692	—
Carbon monoxide...	0·2346	—	—	0·967	1·00293
Carbon dioxide ...	0·2985	0·3316	0·3650	1·529	1·00722
Ethylene	0·3254	0·4052	0·4851	0·9677	—
Nitrous oxide ...	0·3014	0·3362	0·3712	1·5241	1·00651
Ammonia	0·2952	0·3134	0·3318	0·5894	1·01881

Columns I., II., III., contain the true specific heats at the temperatures indicated; column IV. the difference of specific heat at 0° and 200° expressed in percentage of the specific heat at 0°. Columns V., VI., VII., represent the true specific heats in reference to the unit of volume, the specific heat of the unit volume of air being taken as 0·2389; column IX. gives Regnault's proportions of the products of the volumes V and V′, and the pressures P and P′, when P is at the pressure of one, and P′ at the pressure of two atmospheres. Herr Wiedemann thinks that the specific heat determined in these experiments seems to be composed of two parts, the heat caused by work expended on the expansion of the gases in overcoming outside pressure, and secondly, the heat employed in the internal work of the gas itself. He also thinks that attempts to determine the separate parts of the heat of molecular motion, of which the specific heat is composed in constant volumes—of the heat of atoms according to Naumann—and also the attempt to establish simple relations between the two to be still premature, as the alteration of the specific heat with the temperature would cause these effects to have different relations between different temperatures. The author thinks that the alteration of specific heat of the gases with the temperature cannot be explained by the deviation of such gases from the perfect gaseous condition. As an illustration of this he cites the case of ammonia gas, which, although more remote from the state of a perfect gas than nitrous oxide or carbon dioxide, still possesses smaller variations of its specific heat with change of temperature than either of these latter gases.

ACTION OF ANTIMONY PENTACHLORIDE ON CERTAIN ORGANIC SUBSTANCES.—The action of this re-agent on some organic substances has lately been investigated by C. W. Lossner, who gives an account in the *Journ. pour Chimie* of the results he obtained. When chloroform and antimony pentachloride are gently heated together, preferably in sealed tubes to 100° C., the chloroform becomes converted into carbon tetrachloride. Ethyle